A BIOGRAPHY OF FRANÇOIS MAGENDIE.

(*Continued.*)

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THIRD PERIOD, 1821-1855.

JOURNAL AND CONTROVERSY.

The Journal of Experimental Physiology, 1821-1831.

IN Paris during the early part of the last century many new medical and scientific periodicals were added to the considerable number already in existence. The facilities for the publication of work in physiology and experimental medicine seem to have been excellent, and Magendie at first availed himself of them. He contributed to the *Journal Universel des Sciences Médicales,* the *Nouveau Bulletin de la Société Philomatique,* the *Annales de Chimie et de Physique,* the *Nouveau Journal de Médicine, etc.,* but chiefly to that famous periodical edited by Boyer, Velpeau and Leroux in which five of Magendie's monographs appeared. There was, however, at that time no journal devoted exclusively to experimental medicine, and probably it soon appeared to Magendie that there was room for such a publication. Be this as it may, in June, 1821, appeared the first number of his *Journal de Physiologie Expérimentale,* or, as it was subsequently called, *Journal de Physiologie Expérimentale et Pathologique.*[28]

From the title-page of this new journal we learn that at this time Magendie was physician of the central bureau of admissions to the civil hospitals and infirmaries in Paris[29], member of the Philomathic Society, of the Société Médicale d'Emulation, and also member of the medical societies of Stockholm, Copenhagen, Wilna, Dublin, Philadelphia,[30] etc.

The introduction to the first number, which is here partly

[28] *Journal de Physiolgie Experimentale et Pathologique* is the title of vols. II to X inclusive.

[29] Magendie received this appointment July 18, 1818.

[30] To this list were added, in 1824, member of the Royal Medical Society of Edinburgh, and the Reale Accademia dell' Scienze di Torino, and, in 1827, Physician to the Hôspice de la Salpêtrière and Consulting Physician to the Col. royal de Henri IV. In the first few vols. the address of the editor is given, "Rue de Seine, No. 30."

quoted and partly paraphrased, runs as follows: "The two centuries which have just closed have seen the birth and growth of the physical sciences. The task of the early discoverers was twofold. In the first place they had to make the actual discoveries, and in the second they had to overcome the prevailing prejudices. Experimental physiology began with the discovery of the circulation of the blood in the seventeenth century, but it has not progressed with the same rapidity as astronomy, physics and chemistry, perhaps because in this field there have been no geniuses such as Galileo or Newton, perhaps because popular prejudices have been stronger here than in the case of the physical sciences.

"What subject indeed is more fertile in gross errors and absurd beliefs than that of health and disease? Consider the painful disquietude you would produce in the minds of the majority of men if you said to them: *'There are no such things as rheumatismal humour, gouty humour, scabby virus, venereal virus, and so forth. Those things which are so designated are imaginary things, which the human mind has created to hide from itself its own ignorance.'* The chances are that you would be taken for a lunatic just as it but recently befell those who maintained that the sun was immovable and that the earth turned.

"Having taught this science [Physiology] for fifteen years, cultivating it through choice, and having resolved never to separate it from practical medicine, because I regard it as the best guide to follow in a great number of maladies, I believe that I would be doing something useful in publishing a periodic work designed to contain all facts which tend to throw light upon the history of man in health and in disease. I shall receive therefore with acknowledgment and place in the collection which I now announce, all physiological work, all medical researches which, based on precise observations, exact experiments and controlled by a spirit of severe impartiality and love of truth, appear to me to be suitable for illuminating the phenomena of life.

"One advantage which distinguishes the majority of journals devoted to the physical sciences is that they are edited by savants who strengthen such publications by enriching them with their own discoveries, and who are therefore at the same time more competent to judge the work of others. The works which I have published in medicine and physiology give me, perhaps, some claim to the confidence of the public.

"The *Journal de Physiologie Expérimentale* will consist of four numbers per annum, which will appear regularly every three months. Each number will contain six sheets in octavo, more if the material be abundant. Plates will be added when deemed desirable. Subscription, 12fr. per annum."

By way of "enriching" the journal "with his own discoveries," Magendie contributed thirteen articles to the first volume. Some of these, however, were only reprints of his former publications, added, doubtless, to complete the "six sheets in octavo." Such padding soon became unnecessary and the editor's articles became fewer and fewer. That the number should have fallen off is not surprising. Indeed it seems wonderful that he could have found time to contribute at all, since he systematically verified all results of experiments sent to him for publication, as would appear from a foot-note in one of the early numbers of the journal in which he asks his contributors to send in their articles at least one month in advance that he might have time to verify the principal experiments before sending their accounts to the printer.[31] Moreover, Magendie's literary activity was not confined to editing his journal. He re-edited Bichat's "Researches on the Phenomena of Life and Death," 1822, (36) and his "Treatise on the Membranes," 1827, (60) both with commentaries. He also re-edited, with considerable additions, his own book on gravel (1828). In 1825 he published with Desmoulins an "Anatomy and Physiology of the Nervous System" (52). In 1823 and 1828 appeared his two memoirs, on the Nervous System, and on the Brain, and finally, the second edition of his Text-Book and the second to seventh editions of the Formulary.

Thus founded, the *Journal de Physiologie Expérimentale* led a flourishing existence for eleven years. Kergaradec's celebrated memoir on the auscultation of the fœtal heart appeared in the second volume, while among the best known contributers were Andral, Breschet, Velpeau, Poiseuille, Flourens and Bernard. Financially, the journal met with " a success," wrote Magendie,[32] "which I had hoped for only with time; I had even deposited the funds necessary for supporting it for several years. But this precaution was needless; with the second number the expenses were covered. I am doubtless very much flattered by

[31] J: I, p. 101.
[32] J.

this result, but I also sincerely congratulate the friends of science, especially those who wish to see medicine depart from that state of imperfection in which it has been up to our day, and in which many persons through prejudice or other less excusable motives are compelled to maintain it." He adds: "I have never intended that this publication should be of the nature of a financial speculation; I have devoted the profits to perfecting the work."

It would be tedious to give even in outline the contents of Magendie's numerous contributions to the *Journal,* tedious even to give in full their titles, for there were twenty-six of them, not counting editorials and reports; but for the purpose of emphasizing the wide range of his investigations, an enumeration of some of the subjects treated will not be unprofitable.

There were five articles[33] on human and comparative anatomy, six[34] on the special senses and peripheral nerves, eight[35] on the central nervous system, six[36] on the physiology and surgery of the circulation, six[37] on clinical medicine and therapeutics, two[38] on hydrophobia and, finally, a description[39] of two new sorts of gravel. There is, however, one series of articles which is worthy of especial attention, for not only were the experiments therein described of extraordinary importance, but their publication gave rise to a well-known and rather bitter controversy. The discussion of this topic will, however, be reserved for a separate sub-section.

The Bell-Magendie Controversy, 1822 and 1847.

In the third number of Volume II of his *Journal* occurs one of Magendie's most noteworthy contributions to physiology, namely, his article on the functions of the spinal nerve roots (40).

"For a long time," he writes, "I had wished to perform the experiment of cutting the anterior and posterior roots of the nerves arising from the spinal cord of an animal." Having secured a litter of pups Magendie laid bare the cord. "I had then before my eyes the posterior roots of the lumbar and sacral pairs, and, raising them up successively on the blade of a pair

[33] See Appendix: 28, 29, 33, 34, 38.
[34] *Ibid.:* 40, 41, 49, 50, 54, 65.
[35] *Ibid.:* 25, 45, 46, 47, 51, 53, 61, 67.
[36] *Ibid.:* 24, 26, 30, 31, 32, 63.
[37] *Ibid.:* 35, 37, 55, 58, 66, 68.
[38] *Ibid.:* 27, 48.
[39] *Ibid.:* 59.

of small scissors, I cut them on one side. . . . I reunited the wound by means of a suture through the skin and observed the animal. I thought at first that the member corresponding to the cut nerves was entirely paralyzed. It was insensitive to pricks and to the strongest compression; it also appeared to me to be immovable; but soon, to my great surprise, I saw it move perceptibly, although sensibility was always entirely absent. A second and a third experiment gave me exactly the same result. I began to regard it as probable that the posterior roots of the spinal nerves might have different functions from the anterior roots, and that they were particularly designed for sensibility."

Then, with considerable difficulty, Magendie succeeded in cutting the anterior roots. "As in the preceding experiments, I made the section on only one side. . . . One can imagine with what curiosity I followed the effects of this section. The results were not doubtful; the member was completely immovable and flaccid, although it preserved an unequivocable sensibility. Finally, that nothing might be neglected, I cut at the same time the anterior and posterior roots; there was a complete loss of sensation and motion. . . ."

"I am following up these researches, and will give a detailed account of them in the next number. It is sufficient for me to be able to affirm to-day as positive, that the anterior and posterior roots of the nerves which arise from the cord have different functions; that the posterior appear to be more particularly devoted to sensibility, while the anterior appear more especially associated with movement."

As promised by Magendie, the next number contains a second article on the same subject, entitled, "Experiments on the function of the roots of the nerves which arise from the spinal cord."(41) In this communication the author states that, having become curious to know what the effect of cutting the dorsal or ventral roots would be upon the convulsions caused by strychnia, he proceeded to decide the question by means of experiments. These consisted in unilateral section of one or both sets of the nerve roots supplying the hind leg. As might have been expected, in the leg of which the dorsal roots had been cut, the tetanus was just as complete and intense as when these roots were left intact; while, on the other hand, in the leg of which the ventral roots had been cut, the muscles remained lax and motionless.

The publication of these articles at once gave rise to what

FRANÇOIS MAGENDIE,

Engraved by E. Schladitz after the Medallion by David d'Angers.

is known as the Bell-Magendie controversy, for it was asserted that Magendie had done no more than confirm and elaborate the experiments already performed in England by Sir Charles Bell.[40] The basis of the claim made for Bell was a pamphlet printed by him in 1811 and entitled, "Idea of a New Anatomy of the Brain, submitted for the observations of his friends."[41] This work was never intended for general distribution, but was privately circulated among Bell's friends. In it the author described the following experiment:

"On laying bare the roots of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed."[42]

From this experiment Bell concluded that the dorsal and ventral roots have different functions, but in the nature of these functions he was mistaken, for he supposed that upon the ventral roots depended not only motion but also sensation, while to the dorsal roots he attributed the function of control of the growth and sympathies of the parts.

In the interval between his first and second communications, Magendie had been made aware of the existence of this pamphlet, and consequently he was able to add to his second article the extract from Bell's work which has been quoted, and concluded with the following comment with regard to it: "M. Bell, led by his ingenious ideas regarding the nervous system, has been very near discovering the functions of the spinal roots."[43]

The dispute never took on an international character, for although the claim of Bell was taken up in England with considerable vim and venom by John Shaw, one of Bell's pupils, several English authors gave unqualified preference to the claim of Magendie. Mayo,[44] for example, wrote in his text-book, "Mr.

[40] John Shaw stated that M. Magendie "corroborated some experiments which had been previously made in this country; but of the performance of which M. Magendie does not appear to have been aware."—*London Med. and Physical Journal*, 1822, xlviii, p. 343.

[41] *Lond.*, [*1811*]. 36 pp. 8vo.

[42] *Loc. cit.*, p. 22.

[43] J: 1822, ii, p. 371.

[44] Herbert Mayo. "Outlines of Human Physiology. 3 Ed. London, 1833, p. 255. See also: *Med. Times and Gazette*, London, July-Dec., 1855, N. S., xi, p. 558.

Bell was carried by his experiments very near the truth, but he failed at that time to ascertain it. . . . Before Mr. Bell published any other account of the function of these nerves, Magendie had given to the world the true theory of their uses." Magendie himself appears to have preserved a dignified silence, a silence which was, however, misinterpreted even by some of his colleagues, as shall now be shown.

On February 22, 1847, Flourens read before the Academy a "Note concerning the effects of the inhalation of ether upon the medulla oblongata."[45] At the conclusion of the paper Magendie arose. "Our honorable colleague,"[46] said he, "attributes to Sir Charles Bell the discovery of the functions of the spinal nerve roots. . . . It is not without great surprise that I hear him express himself in such a positive manner. . . . If I did not know of his good will, I might be mistaken with regard to his intentions. . . . I beg M. Flourens that when he prints his memoir he will indicate precisely the works of the English physiologist in which the discovery in question may be found described. This is not, I think, too much to require of the impartiality of our colleague."

"In stating that the discovery belongs to Bell," replied Flourens, "I merely followed the common opinion. . . . No one would be more happy than I, could I proclaim that one of the most beautiful discoveries in physiology belongs to France."

"I know," returned Magendie, "that several works on physiology couple the name of Sir Charles Bell with mine . . . but M. Flourens goes very much farther in denying me all participation in the discovery. . . . No doubt M. Flourens has not spoken without having proofs before him. . . . When he has made these known, I shall discuss them. . . . Until that time I maintain that Sir Charles Bell was a complete stranger to the discovery; I declare that my colleague is ill-informed, and his assertions not at all exact."

Flourens replied[47] that he would present his proofs at the next meeting of the Academy and added that Magendie could without doubt refute them since he, Flourens, had based his opinion largely on the attitude of Magendie himself.

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[45] Note touchant les effets de l'inhalation de l'éther sur la moelle allongée; *Compt. rend. Acad. d. Sc.*, Paris 1847, xxiv, p. 253.

[46] *Loc. cit.*, p. 258.

[47] *Loc. cit.*, p. 259.

Accordingly, on the following Monday, March 1, 1847, Flourens opened the discussion with these words:[48] "I beg the Academy to note carefully that I do not seek proofs against my honorable colleague, I seek only to justify my own opinion. . . . In 1833, in the *Journal des Savants,* I expressed myself thus: 'That which we call a nerve is a very complex structure; the simple structure is the nerve fibre. . . . It is only in these fibres that the properties are shown to be distinct and isolated.

" 'It is this which is really the great conception which dominates all the work of M. Bell; it is his experimental analysis, which was not confined to the nerve, but reached successively each of the primitive elements of the nerve, which is the source of all those results . . . with which he has enriched physiology. . . . But on the one hand M. Bell relied too much upon conjectures and deductions drawn from anatomical facts alone. . . . On the other hand he relied too little upon experiment; and thus it is through lack of being sufficiently eager to resort to experiment that he has allowed a French physiologist, M. Magendie, to share with him the glory of one of his most beautiful discoveries, that of the distinct function of the posterior and anterior roots.' That is what I thought, that is what I wrote in 1833. But in 1842 an event occurred which had a great influence on my opinion.

"In 1842 the Academy awarded the prize in experimental physiology to M. Longet, for four memoirs . . . one of which bore the title: 'Memoir on the functions sensory and motor of the columns of the cord and of the roots of the nerves which arise from them.' [49] In this memoir, M. Longet . . . attributed . . . the honor of the idea of the distinct function of the . . . roots . . . to M. Bell: he attributed to himself . . . the merit of the first . . . decisive experiments. Why did not M. Magendie speak?[50] . . . If he had said 'Those are my experiments,' . . . the committee would have paused. His silence was the first cause of my error.

"There is nothing more in favor of M. Bell except a single

[48] Sur la découverte de siége distinct de la sensibilité et al motricité.; xxiv, 316. *Loc. cit.,* p. 316.

[49] Mémoire s. l. fonctions sensoriales e. motrice d. cordons d. l. moelle epinière e. d. racines d. nerfs qui en émanent.

[50] The members of this commission were Magendie, Duméril, Becquerel, Flourens and de Blainville.

fact . . . the following passage in a memoir . . . published in 1811: . . ." [51]

Magendie then spoke[52]: "The . . . facts which our honorable colleague has just cited appear to me to be exact, only he interprets them in a manner which I cannot allow. If . . .I have kept silence during the affair which has just been recalled by my colleague, no one could have interpreted it as a sort of abandoning of my claim; for the report made to the Academy . . . ran verbatim 'that I believed that I ought not to decline, as I could not be judge and party in questions in which I myself was much concerned.' I pass on now to the works of M. Bell. . . .

"It was I who first made them known in France. I analyzed them in my *Journal de Physiologie.* I have set forth their originality. . . . Charles Bell had before me, but without my knowledge, the idea of cutting separately the spinal roots; he had likewise the merit of discovering that the anterior influences muscular contraction more than the posterior. . . . With regard to the establishment of the fact that these roots have . . . distinct functions, that the anterior preside over the movements and the posterior over sensation, that discovery belongs to me . . . and ought to remain as one of the columns of the monument which the physiologists of France have raised since the beginning of the century." *(To be continued.)*

THE DEVELOPMENT OF ANIMAL PHYSIOLOGY.

(Continued.)

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VII.

NOTES ON SOME EARLY PERIPATETICS AND PRAXAGOREANS.

THAT Aristotle developed a system and founded a school of philosophy is familiar historical matter of fact, but it can scarcely be claimed, on a basis of sound evidence, that Praxagoras was the founder of either a definite system or a distinct school of medicine. Nor is the title of this

[51] This was the passage already quoted. Cf. footnote [42].

[52] *Compt. rend. Acad. d. Sc.*, Paris, 1847, xxiv, p. 319.